LETTERS TO THE EDITOR.

**** The attention of scientific men is called to the advantages of the correspondence columns of* SCIENCE *for placing promptly on record brief preliminary notices of their investigations. Twenty copies of the number containing his communication will be furnished free to any correspondent on request.*

The editor will be glad to publish any queries consonant with the character of the journal.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

Wind Pressure and Velocity.

THE importance of an accurate determination of the relation between the pressure and velocity of the wind will be readily recognized. This relation is especially needed by the architect and bridge-builder, since most instrumental determinations are of the wind's velocity. The problem is much more intricate than is ordinarily supposed, and the diverse results obtained by experimenters of great ability show how the determination of the movements and behavior of gaseous media are hedged about with difficulty, and, as already pointed out in *Science* for July 8, the absolute necessity of building up the science of meteorology on a firm foundation of fact rather than theory.

There have been two methods of experimentation: the earliest, with plates rotated upon an arm seldom exceeding 10 feet in length, and, later, by the exposure of plates to direct air-motion. Borda, in 1763, with plates ranging from 16 to 85 square inches area, obtained the following relation,

$$p = (.0031 + .00035\, c)\ S v^2,$$

in which p = pressure in pounds on the plate, c = contour of plate in feet, S = surface in square feet, and v = velocity in miles per hour (this notation will be maintained throughout).

In 1874, Hagen tried most careful experiments with an arm of 8 feet. The velocity ranged from 1 to 3 miles per hour, but the room was so small that at the latter velocity the air was set in feeble rotation. The plates ranged in size from 4 to 40 square inches. He found, as did Borda, that the pressure per square foot increased with the size of the plate. The following is the relation established by him,

$$p = (.002894 + .0001403\, c)\ S v^2.$$

This formula for this relation has repeatedly appeared in print, and each time it has been changed. This is believed to be correct.

Singular as it may seem, these experiments have been almost the only ones quoted in discussions of this question, and yet it is easy to see that they are utterly useless for determining the pressure of a 50-mile wind on the side of a building.

In November, 1886, a few experiments in Washington with an arm of 4 feet, and plates from 16 to 576 square inches area, gave the relation,

$$p = (.0032 + .00034\, c)\ S v^2.$$

The agreement with Borda's results is very interesting.

Afterward, with the same style of apparatus and an arm of 16 feet, the relation found was

$$p = .0034\ S v^2.$$

The velocity of the larger plates was only 4 miles per hour, so that this formula does not help us for greater velocities. It was certainly established that there was no difference in pressure per square foot depending on the size of the plate. Turning to experiments of the second class, we find that Thibault obtains, with plates from 1 to 1.5 square feet area exposed to the wind, the relation,

$$p = .00475\ S v^2.$$

In France, with plates exposed on a locomotive running 44 miles per hour, the relation established was

$$p = .00535\ S v^2.$$

In this case, probably, a slight allowance must be made for the wind with the train.

In the 'Encyclopædia Britannica,' article 'Hydromechanics,' the mean of all the better determinations is

$$p = .00496\ S v^2.$$

We may conclude, 1st, that experiments with whirling arms of less than 16 feet are very untrustworthy; 2d, that we need determinations with rapid, straight-line motion, best obtained, perhaps, by pushing two or three platform-cars loaded with iron in front of a locomotive, exposing the plates on the front car; 3d, the relation $p = .005\ S v^2$ is the most satisfactory yet determined, and does not differ by more than four or five per cent from the truth.

While there has been this great difficulty in determining the above relation, there has been just as much, if not more, in connection with the relation between the velocity of the wind and that of the cups of Robinson's anemometer. Some confusion has arisen from the fact that the standard anemometer in England has 9-inch cups and 24-inch arms, while in our country we have 4-inch cups and 7-inch arms.

It has been determined, by careful experiment in England, that, if the large type of anemometer has a factor of 2.5, then the smaller should certainly have 3.00. Dr. Robinson, after a long research with a whirling machine, decided that the factor (of the smaller instrument probably) should be about 2.5. After trying a few experiments in the open air, however, he changed his view, and decided that the factor should be 3.00. In Washington, with an arm of 16 feet, and a velocity of 12 miles per hour, the factor was found to be 3.00.

Quite recently the Chief Signal Officer, through the kindness of the officials, as a preliminary to carrying on experiments on platform-cars, as suggested above, has had an anemometer placed upon a locomotive of the Baltimore & Ohio Railroad running from this city to Baltimore. Only one round trip has been tried thus far: in the outward trip the velocity of the train was about 20, and returning it was about 46 miles per hour. Allowing for the actual wind, we find the anemometer indication 46 miles going, and 47 returning. The distance was 40 miles, and we may consider that the excess of about 6 miles was due to the heaping up and flowing over of the air in front of the locomotive. All things considered, it seems probable that the factor 3.00 now used in our anemometers of 4-inch cups and 7-inch arms is entirely correct: certainly no change in the present factor can be thought of for an instant. A complete discussion of this question has already been prepared by me, and will appear in October. The other side of this question has been recently presented by Professor Ferrel in the August *American Meteorological Journal.* H. ALLEN HAZEN.

Washington, Aug. 22.

The Formation and Dissipation of Sea-Water Ice.

MR. W. A. ASHE'S opinion on the freezing-point of sea-water, and the conclusions he draws from his experiments, cannot be accepted. The arrangement of the experiment described in No. 228 of *Science* seems to be insufficient. A hole was cut through ice 87 centimetres (2.85 feet) thick. The water within was thoroughly agitated by stirring from below, and during the actual observation slightly agitated. The thermometer was held nearly horizontally, the bulb slighty lower than the rest of the instrument, just below the surface of the water. When the ice-film began to form, the reading of the thermometer was — 2°.9 C. (26°.7 F.), the temperature of the air being — 24°.8 C. (— 12°.6 F.). The greatness of the difference between the freezing-point of the sea-water and the temperature of the air detracts from the value of these observations. The ice is forming so rapidly that brine is included among the crystals: it is even probable that cryohydrates are formed at the surface. On the other hand, the freezing-point of sea-water was not only found by melting sea-water ice, as Ashe assumes, but also by freezing sea-water, and was always found to be between — 1°.6 C. and — 1°.8 C. (29°.1 and 28°.8 F.), according to the concentration of the solution. Mr. Ashe's second remark on this subject in No. 232 of *Science* does not agree with Buchanan's interesting researches on the melting of fresh-water ice in solutions of salts. He has shown by an excellent series of experiments (*Nature*, April 28 and May 5, 1887), that, when sea-water is frozen to the extent of fifteen per cent of its mass, and the crystals so formed are allowed to melt in the liquid in which they have been produced, they melt exactly as they have been formed. If snow or pure ice be immersed in the brine formed by partially freezing sea-water, it melts at the same temperature as the ice which had been formed by freezing the sea-water, so long as the chemical composition is the same in each case.

In a third letter to *Science* (No. 237), Mr. Ashe makes some remarks on the formation and character of Arctic ice. He says, that, as the density of sea-water increases till the freezing-point is reached, ice is not formed at the surface, but at a certain depth. In fact, the